

## Editorial

The recent appearance of the Miller and Brown *A Catalog/Checklist of the Butterflies of America North of Mexico*, published by the Lepidopterists' Society, was a well anticipated event, an event which hopefully would have established a stable nomenclatorial framework for North American butterfly taxonomy for a long time to come. There has been an immediate reaction to this work from several quarters. The critique published below, by Ehrlich and Murphy, has already been widely circulated and formally supported by a significant number of recognized authorities. For reasons which are unclear, the editors of *The Journal of the Lepidopterists' Society* refused publication of the critique, although it was properly first submitted there. We strongly assert that forward movement in any scientific endeavor cannot be achieved without intelligent consideration and discussion. It should, consequently, be made quite clear that this journal, as a scientific publication, can take no position in the matter except to strongly express the desire to have the issue thoroughly aired. We have invited Miller and Brown to rebut the criticism, and they have agreed to do so presently. The basic issue is substantive to all Lepidopterists, not only in this region, but internationally. We cordially extend an invitation for all relevant considered opinion on the matter.

We take this opportunity to emphasize our support of and good relationship with the Lepidopterists' Society. That organization has had a great historical impact on all workers in the field in North America, and continues as the organization binding all Lepidopterists in the region together. We in no way wish to convey any sense of competition between publications of the Society and this journal. We are all working toward the same objective, have an enormous body of information and opinion to disseminate, and will continue to do so in a cooperative fashion.

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## Butterfly Nomenclature: A Critique

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## Introduction

There are two main goals of formal, latinized nomenclature. The first is permitting unambiguous communication about what organisms are being

discussed. The second is expressing, to whatever degree possible, the evolutionary (phenetic or cladistic) relationships among those organisms. These goals are not antithetical, but we contend here that the first has been almost totally ignored lately by many butterfly taxonomists. In part this is because of a confusion of the nomenclature of butterflies (the system of names applied to them) with the taxonomy of butterflies (their classification into groups). And in part it is due to a nearly automatic trend for specialists to inflate the taxonomic rank of the group on which they work.

The basic element of the scientific system of nomenclature is the latinized *binomen*. The binomen is made up of two parts, the generic name followed by the specific name. The binomen is supposed to be a standard, stable label, understandable to workers in all locations—unlike “common” names that vary from place to place. Thus while the Americans call a certain butterfly the “Mourning Cloak” the English call it the “Camberwell Beauty”, the Germans, “Trauermantel”; Swiss, “Sorgmantel”; French, “Le Morio”; and Spanish, “Antiopa”; scientists everywhere should call it *Nymphalis antiopa*.

The preamble to the International code of Zoological Nomenclature states: “The object of the Code is to promote stability and universality in the scientific names of animals.” It is instructive, however, to examine how well the formal nomenclature of butterflies has achieved stability of names.

### Trends in Generic Nomenclature

We have examined the latinized names in seven standard works on Nearctic butterflies: Holland (1898, 1931); McDunnough (1938); Ehrlich and Ehrlich (1961); dos Passos (1964); Howe (1975); and Miller and Brown (1981). In what follows we will often refer to these works by the dates only. We traced the history of only those 1898 names in the Papilionoidea that were still considered to represent valid species in 1961, and were not of questionable residence (“strays” picked up occasionally along the southern border of the U.S.). For example, the 1898 names *Lycaena icarioides*, *Neonympha phocion*, and *Melitaea arachne* (in 1961 *Plebejus icarioides*, *Euptychia areolata*, and *Poladryas pola* respectively) were included, while *Lycaena ardea* (in 1961 considered a subspecies of *P. icarioides*), *Chlosyne chinatiensis* (not described until 1944), and *Dircenna klugii* (doubtful resident) were not.

Changes in the generic names of the 262 species that fit these criteria in 1898 were followed through the subsequent six publications. The average species changed generic name 1.8 times, that is, it has had almost three different generic names in seven standard works. There have, of course, also been a large number of changes in the specific name or switches between specific and subspecific status. Thus three different names in 84

years—a new name every 28 years—is a very conservative estimate of the amount of name changing. Nomenclatural instability, rather than stability, has been the rule.

Consider some examples. Although in the United States the common name Mourning Cloak has remained stable for the entire eighty-four years, Holland first called it *Vanessa antiopa* and then *Aglais antiopa*, whereas everyone from McDunnough onward has called it *Nymphalis antiopa*. Conversely, while everyone from Holland through Howe called the Spicebush Swallowtail *Papilio troilus*, Miller and Brown suddenly declare it to be *Pterourus troilus*. But these have been relatively stable names. *Satyrodes canthus* (1898) became *S. eurydice* (1931, 1938), then *Lethe eurydice* (1961, 1964, 1975), and finally *Satyrodes eurydice* (1981).

Some species have weathered nearly as many generic names as publication appearances. *Thecla m-album* (1898, 1931) has been in *Strymon* (1938), *Panhiades* (1961, 1975), *Eupsyche* (1964), and *Parrhasius* (1981). Others have simply bounced back and forth between genera. *Thecla augustus* (1902, 1931) moved to *Incisalia* (1938), to *Callophrys* (1961) back to *Incisalia* (1964), back to *Callophrys* (1975), and finally back to *Incisalia* (1981).

As the example of "*Incisalia-Callophrys*" *augustus* indicates, continuing disagreement on whether or not to split a genus may inflate the number of generic name shifts. But such cases are only a minor part of a general trend of fractioning genera. Within the sample set of species there has been a 100 percent increase in the number of genera, and therefore a halving of the number of species per genus, during the past 50 years.

This generic splitting occurred in two waves. The 262 species were in 46 genera in 1898, 49 in 1931, 69 in 1938, 71 in 1961, 67 in 1964, 72 in 1975 and 100 in 1981. Thus from 1938 to 1975 the number of genera remained more or less stable. The McDunnough list increased the number of genera some 40 percent over Holland; Miller and Brown ended that period of relative stability with a similar increase. Our contention is that the McDunnough changes were largely justifiable and those by Miller and Brown largely unjustifiable.

In general the name changes made between 1931 and 1938 reflect the fractioning of a few very large, sometimes polyphyletic genera like *Chlorippe*, *Melitaea*, *Satyrus*, *Thecla*, and *Lycaena*, and the recognition of clear cases of priority that did not involve splitting (e.g. *Anaea* for *Pyrrhanaea*, *Polygonia* for *Grapta*). The large increase in the number of genera between 1931 and 1938 is almost entirely accounted for by the splitting of two genera, the blues (then *Lycaena*) and hairstreaks (*Thecla*). The changes between 1975 and 1981, in contrast, reflect in large part, a refusal to recognize subgenera in disregard of both basic goals of nomenclature outlined in the introduction.

Some name changes are almost entirely due to changing "styles" in

splitting and lumping. Consider the case of *Euphydryas editha*. No one doubts that *Melitaea cinxia* is more closely related evolutionarily to *Euphydryas phaeton* than to *Boloria pales* (all are the type species of their genera). But *Melitaea* in the broad sense (including *phaeton*) was considered too large to be a single genus and *Euphydryas* was generally recognized as having generic status by the late 1930s. Thus *Melitaea editha* became *Euphydryas editha*. The latter name remained stable until Higgins (1978), recognizing several evolutionary groups within *Euphydryas*, split up that genus into four genera, concluding that the name of *Euphydryas editha* should be changed to *Occidryas editha*.

Note that splitting *Euphydryas* from *Melitaea* does not make the slightest difference in the amount of relationship communicated by the specific name. For example the first split focussed attention on the relatively close relationship between *Euphydryas phaeton* and *Euphydryas editha* but obscured the somewhat more distant, but no less important, relationship between *Euphydryas editha* and *Melitaea* (= *Chlosyne* = *Charidryas*) *palla*. Whether or not the old *Melitaea* should have been split is a matter of taste, especially since relationships within the Melitaeini (*sensu* Ehrlich and Ehrlich, 1961) are still not well understood. But we tend to think it was useful from the point of view of the communication goal of nomenclature. The advantage to lepidopterists who must frequently discuss species groups within the old *Melitaea* is probably greater than the inconvenience for non-specialists for whom the large, easily recognized *Melitaea* was more useful. As we will discuss below, however, there is no conceivable justification for splitting *Euphydryas*.

The change of *Graphium marcellus* from the genus *Papilio* to the genus *Graphium* was justifiable because the old genus *Papilio* was polyphyletic. *Graphium marcellus* is much more closely related to *Lamproptera curius* than to *Papilio machaon* (or *Battus philenor*). Only by placing all of the Graphiini and Papilionini (*sensu* Munroe and Ehrlich, 1960) into a single genus could *marcellus* be made congeneric with *machaon* without recreating a polyphyletic entity.

### The Rule of Obligatory Categories

In the case of *Graphium marcellus* the need to change the generic name was clear—it could not reasonably remain in the genus *Papilio* (whether it should be in the genus *Eurytides*, a subset of the genus *Graphium* as used here, is a more difficult question). But how does one evaluate the case of the proposed splitting of *Euphydryas*? Here one must somehow balance the needs of different “user” groups.

There is, fortunately, a taxonomic guideline that is very useful in determining where to draw the line. The key point to remember is that taxonomists should not be creating nomenclature primarily for their own use, but as a general tool useful to all biologists. The rule is: *obligatory*

categories above the species level should be kept conservative. What are "obligatory categories"? Categories are ranks in a hierarchic classification. *Obligatory categories* (e.g., Mayr, 1969, p. 89) are those that every animal must be placed in when it is described: species, genus, family, order, class, and phylum. It is especially important to follow the rule with respect to genera. As Ernst Mayr wrote (1969, p. 239): "Splitting is particularly deleterious on the generic level. The generic name is part of the scientific name of an organism and can therefore be employed more advantageously to indicate affinity than can the name of any of the other higher categories."

If this rule is followed then there can be the best of both worlds. Communication with non-specialists is facilitated, because it is normally the obligatory categories that are used for this purpose. But there remains a wealth of non-obligatory categories with which a taxonomist can communicate finer points of difference—superfamilies, subfamilies, tribes, subtribes, subgenera and species groups, to name the ones most used by Lepidopterists.

### Effects of ignoring the Rule

As we have seen, it is primarily generic splitting that has destroyed the stability of the latinized names of butterflies. For example Higgins' (1978) splitting of *Euphydryas* is an attempt to raise what would be reasonable species groups (or weak subgenera) to generic status. He provided no discussion of the basis on which he decided that the relationship between *E. editha* and *E. chalconotus* was more important to communicate in the binomen than that between *E. chalconotus* and *E. phaeton*. Higgins also promoted the weak genus *Euphydryas* to full tribal level, without consideration of what that categoric inflation meant for the taxonomy of the checkerspots (and nymphalids) in general. Presumably the old tribe Melitaeini (and other tribes of the Nymphalidae such as the Argynnini) would have to be raised to subfamily level—implying that the checkerspots are a group as distinct from, say, the fritillaries as the Papilioninae are from the Parnassiinae. It would further require that the Nymphalidae (*sensu* 1961) be divided into eight or so poorly defined families.

Miller and Brown (1981) appear to have seen the absurdity of tribal status for *Euphydryas*, but nonetheless recognized Higgins' daughter genera *Occidryas*, *Eurodryas*, and *Hypodryas*. This, however, leads to something equally absurd within the Melitaeini: considering the differences between *Phyciodes* and *Occidryas* to be of the same order as those between *Occidryas* and *Hypodryas*. In fact *Euphydryas* is an extremely cohesive group, not just morphologically but in its behavior, reproductive biology, chemistry of host plant choice, and allozyme genetics.

Perhaps even more important, *Euphydryas* are now widely used in the research of population biologists. It would be as ill-advised to change the

scope of that generic name today as it would have been to accept the proposal (made some years ago) that the generic name of *Drosophila melanogaster* be changed. It is precisely that sort of nonsense that frequently leads evolutionists, ecologists and others to ignore the important contributions of taxonomists and damages the reputation of taxonomy as a discipline.

Again, *Euphydryas* is hardly an isolated instance. *Pieris rapae*, a name stable for more than a century and enshrined in thousands of papers in the economic literature is now supposed to be changed to *Artogeia rapae*. It is true that the type of *Pieris*, *P. brassicae*, is morphologically and chromosomally a rather unusual species, but the difference between the two could have remained expressed, as it has for decades, by subgenera. It is doubtful that most scientists will accept this change anyway, any more than they will use *Occidryas editha*. The problem then becomes the acceptance of the change by *some* scientists and the confusion that ensues.

Other examples abound in Miller and Brown (1981). *Zerene* was considered a subgenus of *Colias* in the careful revision of the Pieridae by Klots (1933) and in his treatment of *Colias* in Ehrlich and Ehrlich (1961). Should not the grounds on which that judgment was reversed be published? Similarly what is the biological justification for splitting up *Eurema*. It seems a most uniform assemblage, and again Klots found no reason for fragmenting it into several genera. Taxonomies should not be modified by fiat, but only with the publication of thorough analyses backed by data.

Among the least warranted changes in the Miller and Brown list is the resurrection of a series of antique generic names, mostly proposed by Huebner (1819), within *Papilio* (*sensu* 1975). Are we to assume that the judgment of Jacob Huebner, based on the very limited material and information of 150 years ago, should take precedence over that of Eugene Munroe, a modern taxonomist with access to virtually all papilionid species? Huebner was a giant among his contemporaries, but in his time the concept of a genus was far from its modern state. In Munroe's classic paper on the Papilionidae (1961) he states: "...I have failed to find simple and reliable differentiating characters for what appear to be the natural groups of Papilionini. I therefore include all the species in a single genus." And yet everyone is now expected to drop names used since childhood and, for example, call the tiger swallowtail *Pterourus glaucus*.

Huebner's approach to differentiation at the generic level is particularly evident where he (1819) proposed the generic name *Heraclides* for *Papilio thoas*. The three species available to Huebner for consideration and placed by Miller and Brown in *Heraclides* (*thoas*, *crephontes*, and *androgeus*) were actually divided by Huebner into two genera. The superficially almost indistinguishable *thoas* and *crephontes* were placed in *Heraclides* and *androgeus* was split off in *Calaides*. Furthermore, in the

same publication the generic names *Jasoniades* and *Euphoades* were proposed for *Papilio turnus* and *P. glaucus* respectively—two color morphs of the same swallowtail! Both names were buried by the priority of *Pterourus*, which had appeared 40 years previously and been rightfully ignored.

It seems unnecessary to discuss in detail the pointless fragmentation of genera like *Anthocaris*, *Lycaena* (*sensu* 1938), *Boloria*, *Chlosyne*, *Nymphalis*, and *Precis*, and more complex cases where certain splitting may well have been justified (e.g., *Philotes*, *Plebejus*, some Theclini, *Euptychia*). One justification for the recent generic splitting has been that Europeans have done it (see Miller and Brown, 1979, also pp. ix and xvii of Ferris and Brown, 1981 and the nearly identical nomenclature in Miller and Brown, 1981). It is true that there has been a trend in Europe toward having a single species in each genus (which at completion will completely destroy the utility of binomial nomenclature). There was once a trend for European amateurs to name every individual, too (the "aberration" craze). But there is not the slightest reason for American lepidopterists to follow in their footsteps.

Another apparent justification for the recent ultra-splitting seems to have been the mistaken notion (Miller, 1981, p. 54) that the nomenclatural principle of priority is more rigid than it actually is (see Mayr *et al*, 1971) and that "the primary law of taxonomy involves the concept of binominal nomenclature" (Miller, 1981, p. 53). The primary laws of taxonomy have to do with how one arranges organisms into groups, not how one chooses to assign names (or numbers, or symbols) to those groups in order to communicate about the groups and their arrangement. An unfortunate emphasis on names rather than organisms seemingly has led to attempts to recognize the maximum number of genera—a sort of bizarre "conservation of generic names." The resultant trend toward all genera being monotypic, with the concomitant ignoring of subgenera, makes nomenclatural expression of relationships below the tribal level increasingly difficult.

In summary, the Miller-Brown catalogue is a superb historical review and bibliographic tool, and all who study butterflies owe them a great debt for their enormous effort. But their choice of names is simply unacceptable. Their names will not be used by most scientists working with butterflies, including taxonomists, and it will simply make communication even more difficult if some butterfly taxonomists persist in using them on the assumption that to do so is somehow "scientific" or "modern," or that the names are in some way "official." Remember there is no rule that, just because someone has proposed a new genus or resurrected an old one, the judgment must be accepted.

### Recommendations

Unfortunately the Miller and Brown (1981) names have already been

used in two otherwise excellent books directed to laypersons (Pyle, 1981; Ferris and Brown, 1981), and our personal contacts and correspondence indicate widespread distress with the numerous name changes. To avoid further confusion we would like to make some recommendations for stabilizing the nomenclature of North American butterflies:

1). The generic nomenclature in Howe should be adopted, and no changes accepted in it except where required because of clear polyphyly or highly distorted "balance" (Mayr, 1969, p. 241). We recommend this not because we think that the nomenclature in Howe is perfect. It is, however, the widely available major compendium on Nearctic butterflies. It furthermore has a reasonable nomenclature that does not constitute a major departure from other post-1950 works. We are not sure that the splitting of *Speyeria* from *Argynnis* or *Euphydryas* from *Melitaea* was originally justified, but we are certain that to change such widely accepted names now would be foolish. It might be wiser to recognize *Eurytides* as a full genus as Munroe and Ehrlich (1960) did, but *Graphium* can be monophyletic and is widely used. Let it be!

2). Editors should routinely reject any work that suggests generic name changes from those in Howe if it does not contain a thorough *biological* justification for the change—polyphyly or imbalance. Papers should also be rejected unless all changes in rank are accompanied by a discussion of their consequences for the balance of the system as a whole. "Inclusion of all...species within a single genus...fails to recognize their wide generic and specific differences" (Higgins, 1978) and "something is going on with the coppers" (Miller and Brown, 1979) do not meet these criteria. Munroe's 1949 discussion "Some remarks on the genus concept in Rhopalocera" can still provide excellent guidance in this area.

The problem of generic splitting is now so serious that we believe the butterflies are about to go the way of the birds, where it is the *common* names that are used by virtually everyone for communication and the binomens, in addition to being unstable, give few cues to relationships. Do we all really want to use names (Pyle, 1981) like "immaculate green hairstreak," "Cuban crescent-spot," and "western black swallowtail" instead of the shorter *Callophrys affinis*, *Eresia frisia*, and *Papilio bairdii*? Or names like "goatweed butterfly," "question mark," "waiter," and "crimson-banded black," that give no clues to affinity? In an attempt to arrest this trend we are going to petition the Lepidopterists' Society officially to adopt a list of approved generic names based on Howe (1975) to be used in its publications, and to appoint a diverse board to oversee its (hopefully rare!) revision.

Regarding species-level taxonomy, matters are more complicated. We won't go into the evolutionary problems here, except to say that they are much more complex than usually indicated in popular works on butterflies. Those interested in more details can get access to the literature through

Ehrlich (1961), Mayr (1963), Ehrlich and Raven (1969), and Grant (1981). Suffice it to say that splitting at the species level can be justifiable because lumping can conceal important biological differences (e.g., *Lycaena phlaeas* may really be very genetically different from *L. hypophlaeas*). Here again, though, we would tend to be conservative for purposes of communication and not deviate from the treatment in Howe without substantial evidence to justify the change.

We also feel strongly that revisions at the species level of Nearctic butterflies should be accompanied by exhaustive numerical taxonomic analysis and/or careful work on the biology of the organisms. The recent description of *Boloria acrocneuma* (Gall and Sperling, 1980) could serve as a model for the sort of detailed analysis that should support any proposed new specific names in the Nearctic fauna. Whether or not one accepts their judgment, the basis for that judgment is clearly and unambiguously laid out. Our fauna is now so well known that little is to be gained by reshuffling names, or creating new ones, on the basis of genitalic dissections alone. Contrary to mythology, the shape of the genitalia is no magic indicator of taxonomic status (Shapiro, 1978).

On the other hand, little damage is done by splitting in the description of new subspecies. Since the classic work of Wilson and Brown (1953) it has been clear that most subspecies are not biological entities, and they relatively rarely figure in the evolutionary literature. But subspecies names do call attention to certain patterns of phenetic variation, do communicate information about those patterns among specialists, are useful politically in attempting to prevent the extinction of genetically distinct populations, and give pleasure to butterfly collectors. In short they can be conveniently used by specialists and equally conveniently ignored by others.

At the family-subfamily level we contend that the basic treatment in Ehrlich (1958) and Ehrlich and Ehrlich (1967) should be retained. It is the only recent work that considers the entire breadth of the butterflies and the balance of the groups within them in a context of other Lepidoptera and the insects as a whole. The butterflies are an evolutionary uniform group as insects go, and there is no reason for them to be divided into more than 5-6 families. People who revise one group invariably become impressed with the diversity within that group and want to raise the rank of the taxon they are revising. This urge leads to serious imbalances. For example, if the Satyrinae are considered to be a family with *Euptychia* and *Lethe* in separate subfamilies an imbalance is created. That is because there is much more difference between *Papilio* and *Graphium* (only in different tribes) than between *Euptychia* and *Lethe*.

This is not to say that the Ehrlich treatment necessarily should be permanent. Perhaps, for example, the Riodininae should be considered a family or the Libytheidae dropped to subfamily status. But suggested

changes at that level must be accompanied by a consideration of the relationships of all the major groups of the butterflies worldwide and the balance of insect taxonomy as a whole. Any changes should be based on more evidence or better techniques than the Ehrlichs used. Interestingly, a recent cladistic reanalysis of Ehrlichs' data (Kristensen, 1976) found no reason to alter the higher classification beyond dropping the libytheids to subfamily status.

Finally, we would like to reiterate a plea that has been made before (Remington, 1948; Munroe, 1960). It is that research on butterflies be focused much more strongly on studying the biology of these fascinating creatures and much less on continual shuffling of names. Butterflies are increasingly the subject of important research in ecology, evolution, animal behavior, and conservation biology. These are areas in which both professional biologists and amateur lepidopterists have made and can continue to make substantial contributions. Let's keep our nomenclature stable so that we can continue conveniently to talk about it among ourselves and tell the world about it unambiguously.

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